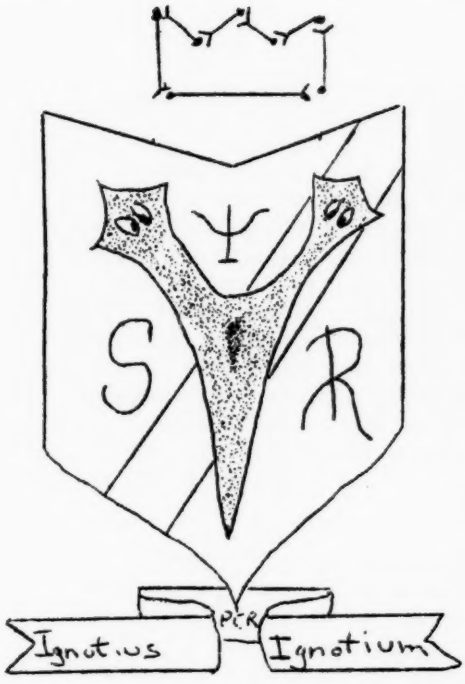

THE WORM RUNNER' S DIGEST

While the angels, all pallid and wan
Uprising, unveiling affirm
That the play is the tragedy ' Man' ,
and its hero the Conqueror Worm.

E. A. Poe The Conqueror Worm

That not a worm is cloven in vain
That not a moth with vain desire
Is shrivell' d in a fruitless fire,
Or but subserves another' s gain.

A. L. Tennyson In Memoriam

I do not want to be a fly!
I want to be a worm!

C. P. S. Gilman In This Our World. A Conservative

You can straighten a worm, but the crook
is in him and only waiting.

Mark Twain

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THE WORM RUNNER' S DIGEST

An Informal Journal of Comparative Psychology

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Department of Psychology
The University of Michigan
Ann Arbor, Michigan

WORMS AND THINGS

*WORMS grow, and so does the DIGEST and its circulation. With this issue we reach for the magical 500 mark and increase the number of foreign countries to which the DIGEST is sent to 14. It is our hope eventually to send this august little publication to anyone who might wish to have it. So, quite frankly, we "crib" mailing lists from any place we can get them. Recently we laid hands on the partial list of members of that amazing group, the Malpighian Society, and in a flash of inspiration, added all these names to our mailing list. As a friend of ours remarked, "The Malpighians are mostly McGill-type biologists," and what better recommendation could one ask for? Again, subscription is free, and we're always pleased to add another name. In fact, as another friend commented slyly, the hard part about the DIGEST is getting OFF the mailing list!

*WORMS change, and so does the character of the DIGEST. Our first issue was put out as a mimeographed answer to dozens of letters from high school students asking us how to condition worms (for more on this front, see below). With the second issue we became "an informal journal of comparative psychology." We hope to continue in this vein, but warn you that our conception of "comparative psychology" is broad enough to encompass almost the entire universe. Thus you will find in this issue an article by a clinical psychologist (shades of Freud!) and a guest editorial which mentions such anethema as Projective Tests (shades of Rorschach!). Someday we hope to change our subtitle to "an informal journal of science." All we need now are contributions from sociologists, historians, physicists, astronomers, etc. Our only promise is this: we try not to be boring!

*HOW OLD are worms? Planaria would, come to think of it, give a sociologist fits since their age is often rather indeterminate. If a worm be hatched from an egg, grow to maturity in two months, then fission into halves spontaneously and regrow to two mature worms in one month, how old are the latter individuals? Three months, or one month old? And if this fission keeps up for an extended period of time, what can one say about age? Or the F_1--F_2 business of generations? Solutions, anyone?

But if the age of worms is indeterminate, the age of worm-runners is not. And it seems to be getting younger all the time. This issue of the DIGEST contains articles by two undergraduates and one high school student, as well as letters which refer directly or indirectly to the work of several other high school students. Nothing could please us more than exciting (we hope) the younger generation to begin its research activities as soon as possible. So with real pleasure we announce the formation of the PENFIELD WORM RUNNERS AUXILIARY, of Penfield Senior High School in Penfield, New York.

The PWRA got its start in the following manner: Miss BARBARA LORENSON, a 13-year-old stellar junior high school student in Penfield, read about the worms in a copy of NEWSWEEK last year. She wanted a Science Fair project and, like many other students, thought the worms might be just the ticket. So, without any contact with us and without reading the benefit (?) of the DIGEST, she set to work and in a matter of two or three months, solved the problems that took the Michigan Group years to solve and managed to replicate much of

our previous research. To no one's surprise, she won Science Fair awards hand over fist. We greatly cherish a color photograph of BARBARA taken at one of the Fairs, showing her equipment (she kept her worms in egg cups) and the award she was given for her efforts.

BARBARA's mother (a most charming woman) teaches at the Senior High School in Penfield, and mentioned her daughter's research efforts to a colleague, MRS. KATHERINE TUCKER. MRS. TUCKER teaches biology, and was looking for a way to interest her students in doing creative research, and the worms seemed an excellent way of breaking out of the cut and dried, cook-book approach to high school biology. Letters flowed back and forth between Penfield and Ann Arbor like snowflakes in a blizzard, and soon MRS. TUCKER was coordinating the experiments of her 55 students with those being performed at Michigan. Under her direction, the students formed the Auxiliary, perhaps the first of its kind anywhere. Such industry and creativity should not go unobserved, the DIGEST Editor believed, so he headed to Penfield to establish direct liaison and is happy to report that the 55 members of the Auxiliary are a sterling and healthy lot and as good at baseball and picnic lunching as they are at worm running. Being young, the members of the Auxiliary are not only more enthusiastic about science and research than many of their world-weary elders, but they also come to science with fewer pre-conceived notions and stereotyped modes of thinking. They also ask more embarrassing questions, the editor observed personally. It is no exaggeration to state that such training is likely to be invaluable to future scientists (whether they actually experiment on worms, rats, electrical circuitry or the biochemical properties of bean sprouts) than all the textbooks in the world. Would that the Editor had had such training! We will be hearing more from the Auxiliary in the future!

As a postscript, may we point out that the Planaria Research Group hopes to visit Penfield en masse this fall, and also we have hopes of bringing MRS. TUCKER and some of her top students to Michigan to visit some time next year. Should any reader know of similar ventures with worms in other high schools, we would appreciate the information.

*CHEAP WORMS? Expensive worms? How much does a worm cost? In the LETTERS section of the DIGEST, MAX BRAVERMAN mentions meeting a high school student who was selling worms at 10¢ each and making a nice profit. Would that conditioned worms could be purchased so easily and cheaply.

One of the problems of conducting University-type, programmatic research in a frontier area of science is that the cost often seems tremendous. How much did the first maze-bright rat cost, or Pavlov's first conditioned dog? THOMPSON and the Editor spent about $3.49 on their first study, but this does not include the price of their own time, the equipment they "borrowed" from the University of Texas Department of Psychology, the "overhead" on the Editor's kitchen (where the study was actually done), nor the price of the worms (which were scrounged, without permission, from the Biology Building fishpond). Graduate research assistants are an expensive lot these days, however, as is the proper equipment for maintaining a lab when such must be purchased rather than "scrounged." Pilot studies, which yield information on what <u>not</u> to do, do not generally yield publications but must be paid for anyhow. And worms that die before the end of the experiment, become sick, or flatly refuse to learn, eat away at research grants without ever providing their share of published glory.

The point of all this cost-accountant's soul searching is this: we sat down the other day and figured how much a single conditioned planarian cost us, and were more shocked than a worm in the training trough. The figure: about $500 each, so far. PAUL CORNWELL has come up with what seems to be a real solution to this problem, along with a method for reducing the tremendous variability in performance that plagues conditioning studies with planaria. We highly recommend that anyone who is actually dealing with worms read his article, which appears first in this issue of the DIGEST.

*IN THIS ISSUE---MARGARET CLAY, AL JACOBSON AND PAUL CORNWELL, being members in good standing of the PRG here at Michigan, are well known to the DIGEST's readers----BOB SOMMER, a clinical psychologist in Saskatchewan, Canada, believes (of all things!) that experimental psychology can be applied to the study of human behavior. He also delights in asking his fellow therapists such sticky questions as, "If you suddenly woke up one morning and found yourself to be a patient on one of the mental wards, how would you establish that you were fit to leave the hospital?" The answers, incidentally, are lulus---- EVA AGOSTON has just been graduated (with honors) from Smith College, where she worked with ELINOR WARDWELL----ditto for JERRY PEIRCE, who worked with our long-time friend (and worm runner) JOHN CAYLOR at the University of California's Riverside Campus----DOUGLAS EWALD received one of the "outstanding" awards (top 5%) at the Illinois State Science Fair and "could have gone on to the National Fair at Indianapolis if Illinois had been registered for it" (Gov. Stratton, please note)=---JOHN HOLLIS has an extensive background in military psychology, but has been working for the past 18 months with WILLIAM A. MASON at Yerkes Labs "on communication, cooperation, and social responses using monkeys and chimpanzees as subjects"----Cover design by REEVA JACOBSON---- And without the services of our Business Manager, ANGELA THOMPSON, who typed the entire issue, there wouldn't have been this entire issue----

*Good reading!

James V. McConnell
Editor

TITLE: Classical Conditioning with Massed Trials in the Planarian

AUTHOR: Paul Cornwell
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Most of the studies done by the University of Michigan Planaria Research Group have employed spaced trials in the conditioning procedure. In most of these studies the intertrial interval has been about 1 min. The usual procedure has been to study one S at a time and administer from 25-30 trials in each daily conditioning session. As most Ss did not reach criterion (23 CRs in any 25 consecutive trials) in less than 150 trials, the minimum time spent with each S was about two and a half hours, spread over 5 days. Many Ss never reached the criterion after even 390-400 trials. The time spent working with each of these Ss was about 7 hours.

During the winter of 1960 Reeva Jacobson found that if 6-8 Ss were placed in the trough at a time and given 100-150 massed trials with short intertrial intervals such Ss gave very high response rates when later tested individually. Jacobson also noticed that the response rate was highest when there was a one or two day delay between conditioning trials and test trials. This procedure seemed very promising in that many animals could be conditioned rapidly. The present study was undertaken in an effort to determine some of the variables affecting conditioning when this procedure is used.

SUBJECTS

Thirty two planaria (D. tigrina) from the University of Michigan Tigrina Stock B were used. All of these Ss were the "descendents" of a single large tigrina. About 2-3 months before the start of the present experiment a single large tigrina was cut into 5-6 sections. As these sections regenerated they were again cut, so that within about 10 weeks there were 70-80 Ss in this genetically homogeneous colony.

APPARATUS

The apparatus used in the conditioning of the planaria has been completely described elsewhere (1, 2).

PROCEDURE

Eight Ss were placed in the trough at a time during the conditioning sessions. Four of these groups of 8 Ss were studied during the initial conditioning sessions. Each conditioning session consisted of 150 3-sec. light presentations and 150 1-sec. shock presentations. For groups A and B the duration of the intertrial interval (ITI) was from 10 sec. to 30 sec., the short ITI condition. For groups C and D the duration of the ITI was from 2 min. to 3 min., the long ITI condition. Groups A and C received the short interstimulus interval treatment (short ISI). In this condition the presentation of the shock (UCS) overlapped the presentation of the light (CS) during the last second of light presentation. Groups B and D received the long ISI treatment. In group B the CS and UCS were always separated in time by 10 to 30 seconds. In group D the CS and UCS were separated in time by 60 to 90 seconds. Thus,

in the long ISI treatment the CS and UCS were not paired. In this long ISI condition the CS and UCS were not strictly alternated, but no more than two CSs or two UCSs were presented in a row.

Training was carried out in three blocks of 50 trials each. A 5-min. interval separated each block of trials. During these initial 150 trials no attempt was made to record responses. During these trials Ss were kept off of the electrodes at the ends of the trough by being re-directed with water currents squirted from a pipette.

Following the initial 150 trials all Ss received 25 test trials. Only one S at a time was studied during these test trials. Each test trial consisted of a light-shock pairing; the duration of the CS presentation was 3 sec. and the UCS was presented during the final second of CS presentation. The intertrial interval during the test trials was the same for all Ss, with the minimum ITI being 1 min. During the test trials all responses occurring during the initial 2 sec. of light presentation were recorded.

Test trials were run after one of two different delay conditions. Four Ss from each of groups A, B, C, and D were given a 1-4 hr. delay period in a dark room between the 150 conditioning trials and the block of 25 test trials. The other half of the Ss in each group had between 19 and 25 hrs. delay between conditioning and test trials.

It should be noted that group A, the condition of paired CS-UCS and short (10-30 sec.) ITI, is very similar to group E of Thompson and McConnell (3). In the present study 8 Ss were trained at a time while in the Thompson and McConnell study Ss were studied individually.

EXPERIMENTAL DESIGN

A three way analysis of variance design was used, with the main variables being 1) the length of the ITI, 2) the length of the ISI, and 3) the delay between completion of the conditioning trials and the start of the test trials. Two conditions were used on each dimension, making a total of eight groups.

RESULTS

The mean number of CRs made by each group during the 25 test trials is given in Table I. The individual responses rates of Ss in the various groups are given in Table II. An initial inspection of the data shows that Ss tested after a long delay show a higher rate of response than Ss tested after a short delay.

A Bartlett's test for homogenity of variance run on this data gave a X^2 of 27.5. With 7 df a X^2 of 20 is significant at the .005 level. Although with variances this heterogeneous it might be argued that an analysis of variance should not be done, such an analysis was undertaken in order to give possible leads for further experimentation. The results of this analysis of variance are given in Table III. These results show that response rate is a function of the length of delay between massed training and later testing. It is also interesting that under these experimental conditions the rate of response is not a function of the temporal contiguity of the CS and UCS.

TABLE I

Mean Number of CRs Made During the 25 Test Trials

Rank	Mean Number of Responses	Group	ISI.	Conditions ITI	Delay
1	21.5	B-1	long	short	long
2	20.5	A-1	short	short	long
3	19.3	C-1	short	long	long
4	18.8	D-1	long	long	long
5	18.5	C-s	short	long	short
6	13.5	B-s	long	short	short
7	13.0	D-s	long	long	short
8	9.0	A-s	short	short	short

TABLE II

NUMBER OF CRs IN 25 TEST TRIALS

Short (10"-30") Intertrial Interval During Conditioning

	PAIRED CS-UCS			UNPAIRED CS & UCS	
(Grp.A)	Test Trial CRs		(Grp.B)	Test Trial CRs	
Animal	1-4 hr.delay	19-25 hr.delay	Animal	1-4 hr.delay	19-25 hr.delay
P_1	13		U_1	21	
P_2	12		U_2	19	
P_3	8		U_3	14	
P_4	3		U_4	0	
P_5		19	U_5		20
P_6		21	U_6		23
P_7		17	U_7		20
P_8		25	U_8		23
Mean	9	20.5	Mean	13.5	21.5
S.D.	3.9	2.9	S.D.	8.2	1.5

Long (2'-3') Intertrial Interval During Conditioning

	PAIRED CS-UCS			UNPAIRED CS & UCS	
(Grp.C)	Test Trial CRs		(Grp.D)	Test Trial CRs	
Animal	1-4 hr.delay	19-25 hr.delay	Animal	1-4 hr.delay	19-25 hr.delay
P21	14		U21	21	
P22		22	U22		17
P23	17		U23	22	
P24		18	U24		18
P25	20		U25	2	
P26	23		U26	7	
P27		21	U27		17
P28		16	U28		23
Mean	18.5	19.3	Mean	13	18.8
S.D.	1.7	2.4	S.D.	8.7	2.5

TABLE III

Results of Analysis of Variance

Source	Sum Squares	df	Mean Square	F	p
Between ISIs (paired-unpaired)	.125	1	.125	-	not sig.
Between ITIs	12.5	1	12.5		
Between Delays	338.0	1	338.0	10.97	.005 > p
Interaction ISI x ITI	66.125	1	66.125	2.16	p > .05
Interaction ISI x Delay	1.125	1	1.125		
Interaction ITI x Delay	84.5	1	84.5	2.74	p > .05
Interaction ISI x ITI x Delay	36.1	1	36.1	1.17	p > .05
Within Groups	739.5	24	30.81		
Between Cells	538.5	7			
TOTAL	1278	31			

In the present study no responses were recorded during the first 150 trials. In the Thompson and McConnell study no trials were run beyond trial 150. In order to compare the results of the present study with those obtained by Thompson and McConnell, two predicted frequencies of response for the Thompson and McConnell group E have been made. The lower bound for the predicted frequency of response between trials 151 and 175 is 10.9, the frequency of response for the preceding 25 trials. The upper bound for the predicted frequency of response between trials 151 and 175 is 12.5, the performance asymptote estimated by Thompson and McConnell. Thus, the predicted range of frequency of response for the Thompson and McConnell group E between trials 151 and 175 is from 10.9 to 12.5. In the Thompson and McConnell group E the variance of the number of CRs between trials 126 and 150 was 7.4. It will be assumed that this would be a reasonable estimate of the variance between trials 151 and 175.

In the present study the paired CS-UCS, short ITI, short delay group (group A-s) was run in an attempt to replicate the conditions of Thompson and McConnell's group E. The variance for the number of CRs between trials 151 and 175 for this group of the present study was 15.6. An F-test shows the variance estimate based on this sample variance to be significantly different from the variance estimate based on the sample variance of Thompson and McConnell's group E. The above F is significant at the .01 level.

If a t-test was run to test the difference between the predicted mean for the Thompson and McConnell group E and the obtained mean for group A-s of the present study, and the result of such a t-test was significant, then there would be no way of telling how much the difference in variances had contributed to the results of this t-test. However, in the present case we would like to

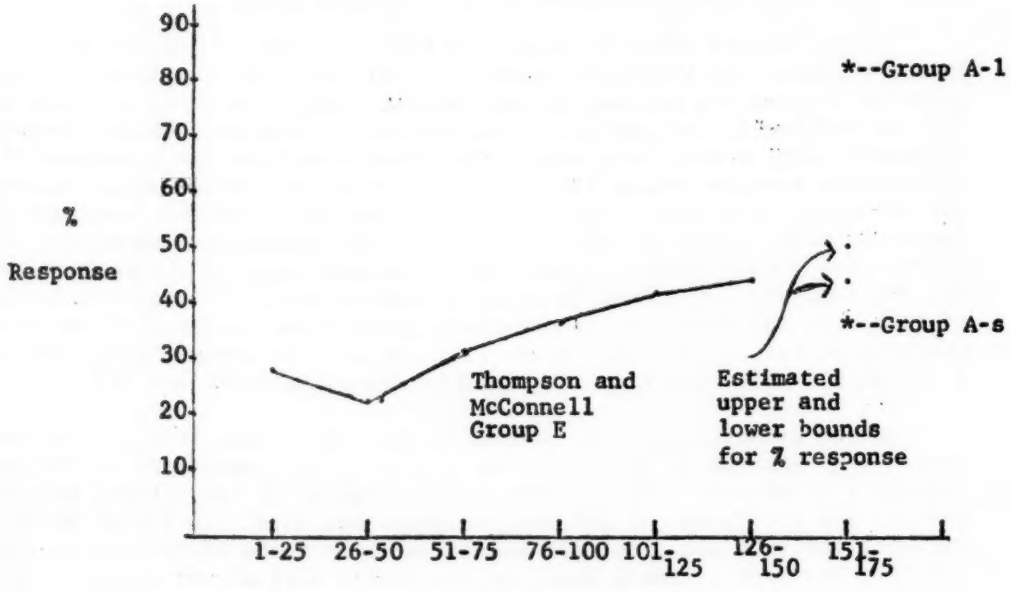
show that there is no significant difference in frequency of response between the above groups. Thus, an insignificant t would indicate that there is no significant difference between the means. Therefore t-tests have been run.

The mean frequency of response between trials 151 and 175 for group A-s was 9.0. Comparing this mean with that of the predicted upper bound for trials 151 to 175 of Thompson and McConnell's group E (12.5) gives a t of 1.44. This is not significant at the .05 level. Comparison with the predicted lower bound for trials 151 to 175 of group E gives an even smaller t.

If, instead of comparing the short delay condition (A-s) with the predicted limits for the Thompson and McConnell group E, the long (19-25 hrs.) delay condition (A-l) is compared with group E there are much different results. The mean response frequency for this long delay group between trials 151 and 175 was 20.5. The variance for this group was 8.8. Comparison of this mean with the upper predicted bound (12.5) for the group E mean between trials 151 and 175 gives a t of 4.1. This is significant at the .01 level. Comparison with the predicted lower bound for trials 151 to 175 gives an even larger t. The above comparisons are summarized in Figure I.

Figure I

Comparison of Group A
with Thompson and McConnell's Group E



DISCUSSION

Possibly during massed training there are two main determinants of response rate: 1) sensitization or conditioning and 2) fatigue. Both of these determinants may be functions of the length of the ITI, the sensitization or

conditioning variable giving an increased probability of a CR with a short ITI, the fatigue variable giving a decreased probability of a CR with a short ITI. With massed training the conditioning or sensitization may last for a considerable length of time while the recovery from fatigue is more rapid. Such a scheme might account for the higher response rate with a one day delay between conditioning and testing than with a 1-4 hour delay between the two sessions.

It was expected that the temporal contiguity of CS and UCS would be a major factor in establishing the CR. The results of the present study would seem to indicate that under conditions of massed training this is not so. Perhaps temporal contiguity of CS and UCS is an important determinant of response probability, but in the present study no group was run with a long enough ISI to prevent the establishment of CRs.

The results of the present study would seem to suggest that shock increases the sensitivity of planaria to respond to light. However, in this study no control groups were studied in which Ss are given several shock trials and then exposed to light. In the Thompson and McConnell study a shock control group (Group SC) seemed to show that shock did not sensitize planaria to a light stimulus. However, until it can be shown that the probability of a response to light is a function of the previous temporal contiguity of the light with a UCS it is a tenuous statement to say that the planarian is being classically conditioned rather than sensitized under conditions of massed training.

SUMMARY

Thirty two planaria were given 150 massed trials of light and shock. Following the conditioning session Ss were tested for the presence of the CR. A 2 x 2 x 2 analysis of variance design was employed, the main variables being 1) length of ITI 2) length of ISI and 3) delay between conditioning and testing sessions. Results of the analysis of variance showed the probability of a CR during the test session to be a function of the delay between conditioning and test sessions, with the long delay groups showing the greatest numbers of CRs. Under the conditions of the present experiment the probability of a CR during the test session was independent of the temporal contiguity of CS and UCS during conditioning. The possibility that with massed training shock sensitizes planaria to light is suggested.

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TITLE: Preliminary Report on Project "Burrow"

AUTHOR: John H. Hollis
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This project was undertaken after obtaining unsatisfactory results running meal worms (Tenebrio molitor) in mazes. Stimulating the worms sufficiently for repeated runs of the maze in a minimum of time appeared to be difficult, with unpredictable results. Removing the worms from the culture provided another, but interesting problem. The worms would readily burrow into the medium (All Bran) at a rather rapid rate when their container was opened. It was apparent that with increased stimulation (illumination and disturbance of the medium) the worms would become highly active and rapidly burrow into the culture. After a few preliminary tests it was clear that the worms were good subjects for study in the vertical plane and that more than one variable was involved in the rate of burrowing.

The purpose of the project was to investigate the effects of two variables on 'time-to-burrow'. The stimulus dimensions selected were burrowing surface and illumination.

The experimental design followed a 3 x 3 Graeco-latin square for repeated measures, replicated five times. A total of 15 meal worms were run, three for each replication. Each worm was run successively on three different surfaces each paired with one of the three illumination levels in counterbalanced order.

The apparatus consisted of three plastic containers 12 x 9 x 6 cm. placed 47 cm. under the illumination source. The containers were filled to a depth of 4 cm. with the specified burrowing material. The three burrowing materials were: (a) All Bran cereal, (b) baking flour, (c) uncooked white rice. The three illumination sources consisted of: (a) shielded 7 watt red lamp, (b) 25 watt white lamp, and (c) 300 watt reflector flood lamp.

The experimental procedure was as follows: (a) The dark adapted worms (30 min. dark adaptation) were removed from their containers, one at a time and placed on the appropriate burrowing surface illuminated only by a shielded 7 watt red lamp, (b) on contact with the burrowing surface a stop watch was started and the appropriate lamp turned on, (c) the timer was stopped when the worm had burrowed into the material out of sight (criterion, worm below surface plane of material), (d) S removed from burrowing material and placed in container for five minutes of dark adaptation, (e) same procedure followed for the two remaining trials, and (f) at completion of the third trial S's length was measured and recorded.

In Table I the mean burrowing times are presented in rank-order for the nine composite stimulus conditions. The range for the nine ranks is 100 sec., most likely a significant difference. However, there are several ranks that do not appear to differ significantly. (For Table I, see next page.)

Performance was best in All Bran under the 7 watt shielded red lamp. The bran ranked first, second, and third for burrowing material depending on type of illumination. Burrowing performance was poorest under the 300 watt white lamp in flour. Between these two extremes there were considerable differences

for various material and illumination combinations. These results suggest that the materials used were differentially effective: (a) All Bran, providing a light coarse medium that normally lies in such a manner that tunnels occur almost naturally, (b) Rice, much heavier than bran. Its small kernels tend to lie tightly together and to offer a difficult surface for penetration, (c) Flour, it forms a dense light-reflecting surface, powdery and without air spaces or material to grip. It would appear that for burrowing, the All Bran offered the least resistance and permitted burrowing with the least effort. However, no attempt was made to rule out the possibility of differential stimulation of the chemical senses by the burrowing material. As an added note, additional observations suggest that perhaps touch (ventral surface contact with medium) may play a part in initiating and maintaining burrowing activity.

Illumination appeared to have both a facilitative and inhibiting effect on burrowing time. The 7 watt red lamp appears to have provided the best source of illumination. Then follow the 25 watt and 300 watt lamps. The 300 watt lamp may tend to inhibit burrowing, especially in the flour. This may in part be due to a resultant of direct and reflected light impinging on the worm. However, it is possible that an increase in temperature from the large lamp might interact with the light intensity to inhibit burrowing.

The results reported are tentative and based on only an N = 5 for each material-illumination pairing. A statistical analysis has not been performed for this reason. It is hoped that in the future more data may be added in support of this preliminary investigation.

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Table I
Rank-order of mean burrowing time (N=5, per rank)*

Rank	Mean time (sec.)	Material	Illumination
1	19.4	All Bran	7-Watt red
2	36.2	All Bran	300-Watt white
3	40.4	All Bran	25-Watt white
4	54.6	Rice	7-Watt red
5	69.4	Flour	25-Watt white
6	78.2	Flour	7-Watt red
7	79.2	Rice	300-Watt white
8	89.4	Rice	25-Watt white
9	119.4	Flour	300-Watt white

* (a) Average length of meal worms = 2.85 cm.
(b) Average temperature of air at surface of burrowing material at beginning of each replication = 78° F.

TITLE: Project "Glowworm" Revisited*

AUTHOR: Margaret L. Clay
Planaria Research Group
University of Michigan

To refresh your memories about the first episode in our new serial thriller, a short synopsis follows:

As the curtain came down last time, little Nell was tied to the railroad tracks by the villain, just as a fast train.....

(Oops, wrong manuscript!! The boss will scalp me. - Ah, here it is... Now, to begin again:)

The first of the "glowworm" studies (1) was exploratory, primarily an attempt to determine the appropriate x-ray dose for investigating effects of radiation on conditioning and retention of the conditioned response in planaria. With such an approach, we neither expected to be able nor found ourselves able to derive highly specific conclusions. The lethal threshold dose was located somewhere between 500 and 1000 roentgens for our irradiating conditions (250 kvp, 20 ma., HVL - 0.875 mm. Cu., tsd - 40 cm.). This value differed considerably from that in earlier studies by other investigators but could be explained as due to the use of much higher energy x-rays than were available at the time the earlier studies were done. We also had evidence that regeneration is temporarily inhibited by the lower dose but returns to normal in about six weeks.

To more closely narrow the known range between lethal and non-lethal doses, we performed a second experiment very similar to the first, except that we used ten groups of worms, varying x-ray dose from zero to 900 r. in 100 r. intervals (the dose to each group given acutely at a rate of 200 r. per minute as before).** Tables I and II show the main results from the two studies.

TABLE I (Study I)

Dose (r's)	0	520	1040	1560	2080	2600
No. in group	8	15	15	15	15	15
% died	37.5	53.3	100	100	100	100
% tails dropped 3 wks. after irradiation	62.5	0	0	6.7	0	0
% tails dropped 6 wks. after irradiation	189.5	62.5	0	6.7	0	0
% tails died	0	0	-	100	-	-

* This research is being supported by a grant from the Atomic Energy Commission.

** We gratefully acknowledge the cooperation of the University of Michigan School of Public Health and R. J. Augustine, Research Associate in Environmental Health, in allowing us access to and assistance with their x-ray facilities.

TABLE II (Study II)

Dose (r's)	No. in group	% died	% tails dropped 30 days after irradiation	% tails died	% tails regen. 3 wks. after irradiation	% combined worms and tails 2½ mos. after irradiation
0	12	0	108.3	0	81.8	241.7
100	13	0	169.2	0	86.7	223.1
200	12	0	83.8	0	75.0	141.7
300	13	0	138.5	0	86.7	238.5
400	17	5.9	94.1	0	80.0	170.6
500	13	7.7	69.2	0	42.9	269.2
600	16	37.5	56.3	0	25.0	156.3
700	13	69.2	46.1	50.0	16.6	107.7
800	12	100	33.3	100	0	0
900	13	100	7.7	100	0	0

It appears fairly conclusive when both studies are considered that the LD50 for the planarian under our conditions is between 600 and 700 r. (acute whole body irradiation), and that doses below 500 r. neither kill the animals nor inhibit regeneration observably. In Study II, none of the animals showed signs of illness for 11 days after irradiation, and then all sickness was of the second type observed in Study I (i.e. burn marks, malformed heads, humped pharynxes, and reduced motility). This supports our conclusions that the early sickness in Study I was due to increased susceptibility to a disease already present and was not really radiation sickness. The later illness in Study I appeared in the remaining, healthy-looking worms after approximately two weeks, just as did the only sickness in the second study, and with the same symptoms.

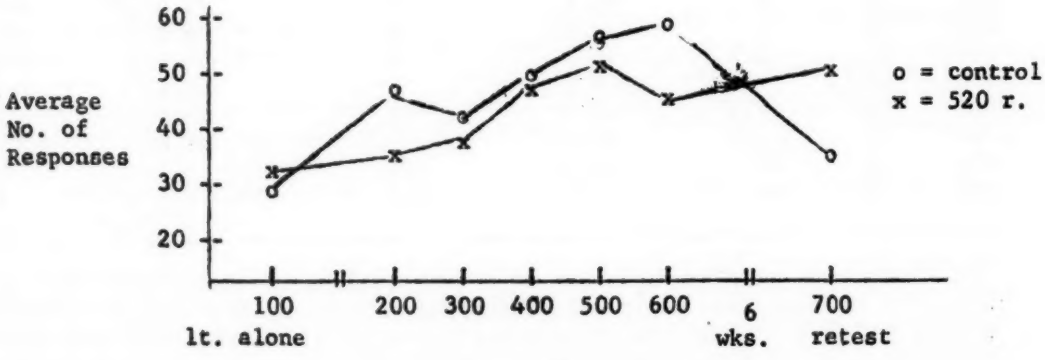
In both studies all animals showed radiation sickness in the higher radiation groups and eventually died. But with increasing frequency as dose diminished, animals and tails from the 700, 600, and 500 r. groups showed sickness but recovered and regenerated. No signs of illness showed in any groups receiving 400 r. or less of radiation, though one 400 r. animal died almost four weeks after irradiation. Since we occasionally lose an animal even without radiation, this event may have no significance at all.

Those of you who read our first report may remember that we ran five animals from the 520 r. group and five control animals in a supplementary pilot study on conditioning. Since we were primarily investigating lethality of dose, we didn't start conditioning these animals until all observable signs of radiation sickness had disappeared. It was therefore to be expected that we could pick up only long term, more or less permanent effects of radiation on learning, provided any existed.

As expected, the results of this exploration were not sensational, but I'll pass them on to you for what they're worth. We ran the animals 25 trials per day in our conditioning troughs, pairing 3 seconds of light with shock accompaniment in the last second (from a standard inductorium). [See McConnell, Cornwell, and Clay (2) for a detailed description of our apparatus.] In the first 100 trials, the shock was omitted in order to establish a base line of response for each worm to the light alone as a neutral stimulus. Then followed 500 trials of the paired light and shock. After a six week interval, all worms were run an

additional 100 trials with light and shock to test retention of the conditioning occurring in the initial run.

The following graph gives a pictorial impression of the comparative performances of the two groups. Points marked are average responses per block of 100 trials (4 days).



It can be seen that the general tendency was for the control group to condition rather quickly and hold this conditioning throughout the initial run while the irradiated group learned more slowly. The irradiated worms, however, showed better retention in the retest block of trials after a six week period of rest. I hasten to add that our N was small, our variability high, and none of these differences are significant. At best, they can only guide us in designing further studies. It is not, however, inconsistent with our findings on regeneration that more subtle effects of radiation, if we find them, will be in the direction of inhibition. That is, if conditioning facility depends at all on neural cell growth or metabolism, a lessened efficiency of these processes might slow the acquisition of the conditioned response. By the same token, if permanent, it might also tend to retard extinction occurring during the six week rest interval due to the learning of competing responses to light during this period (i.e. as when animals were fed, bowls were cleaned, etc.)

We hope to test this hypothesis, among others, somewhat more reliably in our next study, now under way, in which we're subjecting different groups of worms to massed conditioning before, immediately after, and 10 days after sub-lethal radiation, then reconditioning the animals individually after a ten day rest period. So, like all good serial thrillers, we'll end this episode here - to be continued in the next installment....

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TITLE: Personal Space*

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Houses should be designed from the inside out rather than the outside in. This means that architects need information on how people use space. It is surprising how little is known about people and space in comparison to what is known about animals and space. Perhaps one reason is that if too little or too much space is provided for animals in zoos or circuses, they soon become ill and die. Since zoo animals are valuable property, the zookeeper strives to learn the conditions suitable for his animals. Unfortunately this consideration does not apply to human beings. Whether they are put in closet-sized rooms or castles, we don't know what happens to them. If they become ill and die, it is blamed on a weak heart, poor constitution, or inadequate diet. The last thing that is ever blamed is the amount of space available.

I do not intend to suggest that human beings are like animals or vice versa. I do wonder, though, whether any of this research devoted to animals and space has any relevance to humans and space. For example, we know that most wild animals have a specific "flight distance" beyond which the animal will flee if intruded. Hediger, an animal biologist, found that the wall lizard can be approached to within a few yards while the crocodile makes off at fifty. The sparrow and crow have very small flight distances in comparison to eagles, deer, and chamois.

As far as we know, there has been no research to discover whether man has a flight distance. It seems obvious that people feel uncomfortable when others stand too near them. I have often tried what I call a "waltz technique" while speaking to others. Sometimes I slowly move towards the other person until he gradually starts retreating. I've also done the reverse, moving back, while talking to someone. Soon I found that he would gradually, almost involuntarily, begin moving towards me. Modifications of this technique can be quite useful at parties for maneuvering a potential customer or attractive blonde into another room. One simply invades her personal space and forces her to the right or left. This is the same method used by lion tamers.

For the past few years I have been interested in studying the personal space of human beings. In doing this work, I found it necessary to make a distinction between <u>personal space</u> and <u>territory</u>. The idea of territory is very important to anyone who has ever worked with animals. In their natural habitat, almost all animals have territories. The most thorough research on territories was done by Eliott Howard with birds. He emphasized the role of territory during mating and rearing offspring. He observed the male finches quickly isolated themselves from one another and reigned supreme in small areas. Elton noted that no more than one family of African Lions could be found inhabiting a single district. Territorial boundaries vary considerably from season to season. For the majority of woodland birds, territory is confined to spring and summer. Badgers will permit trespassing by other badgers, except when the cubs are small and possibly in the late autumn when food is scarce.

*Published in <u>The Canadian Architect</u>, Feb., 1960, pp. 76-80. Reprinted by permission.

In recent years, a lot of publicity has been given to the use of territory by adolescent groups. Often referred to as "turf" a gang's territory is where the members feel safe and where they will attack intruders. Within the last month, there was a newspaper report of a young boy who shot another because the latter dated a girl on his gang's territory. Salesmen of course have territories, as do people in housing developments.

Personal space is a different concept and refers to the distance that an animal customarily places between itself and other animals. The distance may vary from species to species, and from individual to individual. A person may feel comfortable when his child stands near him but be uncomfortable if an adult male occupied the same place. A chicken would not be disturbed at the approach of a robin, but a hawk would upset her.

Personal space can be distinguished from territory in several ways. The most important difference is that personal space is carried around while territory is relatively stationary. An animal or man will usually mark the boundaries of his territory so that they are visible to others, but the boundaries of personal space are invisible. Personal space has the body as its center, while territory does not. Often the center of a territory is the home of the animal or man. Animals will usually fight to maintain dominion over territory but will withdraw if others intrude into personal space.

My first research into personal space took place in the staff cafeteria of a large mental hospital. The cafeteria was 36 x 68 feet and contained 13 tables (36 x 72 inches). Each of these tables consisted of two 36 x 36 inch tables placed together surrounded by eight chairs. At dinner the employees secured their plates at the front, went in line, were served, and then sat at whichever table they chose. Over a two-month period, my wife and I made periodic observations of conversation at the tables. We were interested in the relative positions of people who were conversing. For example, did conversation occur between the head and foot positions? Or between people seated alongside one another? When we began the study, we had no specific ideas about what we would find.

The results showed, not so surprisingly, that most of the conversation took place between people seated in adjacent chairs. We also found that the corner positions were the loci of most of the interactions. There was far more conversation at the corners than there was across the tables. These results could mean two things, either the corners themselves spark the interactions, or that people who want to converse will sit at the corner positions. To learn more about this, we asked people to enter the cafeteria and discuss certain topics, while we observed where they sat. We found that people in pairs or threes who wanted to discuss a topic would choose the corner positions. We also found this when we repeated the experiment in another cafeteria with different sized tables.

In other experiments, we employed a "decoy", someone who was a confederate of mine and who was <u>already seated</u> at a particular chair before the person entered the room. Another person was then asked to go inside the cafeteria, sit down, and discuss a topic with the seated "decoy". We used both male and female "decoys" and male and female discussants. We found gross differences in the ways that males and females would sit opposite decoys. Females would sit much closer to a female decoy than they would to a male decoy. Males tended to sit opposite decoys of both sex. These results parallel other observations about the differences between the sexes in this respect. Females are often seen

holding hands or kissing, while this is comparatively rare among males in the Canadian culture.

We also tested groups of three, four, and five people at the tables. Some of the groups had "leaders" while others did not. We found that the leaders gravitated to the end positions and that other people would try to face the leaders. It was more important for the other group members to face the leader than it was to sit on either side of him. The usual concept of the leader in the center surrounded by his followers was not supported in our research. It seemed more important for them to face him than to be on both sides of him. In groups with single leaders, only one end of a table was used. The people fanned out along one end of the table. We hypothesized that if both ends of the table were occupied, this would tend to divide the group into two factions. With this type of table, the best place for a leader to sit would be the side-center position. From here he could reach all the people in the group. I recall a friend complaining that whenever he sat at one end of a table, the most vocal member of the group always sat at the other end, too far away for a comfortable discussion. When I suggested that it would be easy to reach all the people from the side-center position, he felt that he would lose status if he gave up the head position at the table.

We also conducted a series of experiments to learn what de N. Schroeder called the "arc of comfortable conversation". We wanted to find out how close to each other people would sit when they wanted to converse. Our method was straightforward and we used some of the principles that we learned in our other work. We took two couches and arranged them one opposite the other at various distances. We then asked pairs of people to enter the room, sit down, and discuss certain topics. We wanted to learn whether the people would sit side-by-side or opposite to one another. We reasoned that people would begin sitting side-by-side when the distance between the two couches became too great for comfortable conversation. The results are shown in Table 1. This Table reveals that when the couches were three and a half feet apart, the people began to sit side-by-side. It should be noted that the distance between the couches were not necessarily the distance between the people. In actual fact, the people's heads were about one foot back on each couch. In other words, the transition from opposite seating to side-by-side seating came when the people were 5½ feet apart. This distance we conceive of as "the arc of comfortable conversation" under the conditions we used. When we repeated this work in another room with chairs instead of couches, similar results were found.

Table 1

Seating at Various Distances

Distance between couches	Number of pairs sitting: Opposite	side-by-side
1 foot	8	4
2 feet	11	2
3 feet	12	6
3½ feet	1	8
4 feet	1	11
5 feet	2	0
6 feet	0	5

At another occasion we ushered people into a small dormitory containing eight beds, four on either side of the room. When we asked pairs of people to choose which of the beds they would prefer to occupy if they had to sleep in the room, we found a marked preference for the corner beds. This result parallels what we had found when we tried to shift the chairs around on a ward containing many older people. We found that whenever we tried to move the chairs away from the walls, they soon returned on the next day. The people like to have their backs against something. Whether this is a fundamental trait of human nature I don't know. We know that the hero of the Western movie likes to sit with his back to the wall and many wild animals will sleep with their backs to trees. In biology the attraction of some cells to a solid surface is called "thigmotaxis".

Last year at the Geneva Foreign Ministers' meetings, the issue of the shape of the tables to be used attained world-wide prominence. The Russians insisted on a square table, while the Western powers wanted a round table. The issue was finally settled by the adoption of a round, hollow table with separate rectangular tables for the East and West German representatives. Whether this solution aided the Conference, I do not know, but the matter was important enough to delay a meeting of Foreign Ministers for several hours. Presently we are attempting to learn something of the characteristics of round and square tables. It is apparent that the arc of the table has a great deal to do with its effects on conversation. With a round table of large diameter, interaction would be limited to people in adjacent chairs, while at tables with smaller dimensions, people could converse across the table. Eventually it may be worthwhile to experiment with freeform and hexagonal tables. Tables with straight sides, such as rectangular or octagonal tables, provide the person with clearer boundaries to his territory. He knows where his neighbor's territory ends, and his own begins. With round tables a person is less certain about the limits of his territory.

This work has made me aware of the effects of room geography. At private parties or meetings I have found myself observing the way people were arranged and how close they stood to one another. I recall one party at which people sat in chairs along the walls of the room, and the two sides of the room were eight feet apart. I noticed that during the entire evening there was <u>not one</u> conversation between people sitting on different sides of the room. All conversation took place between adjacent chairs. The angle at which people stand from others is another matter that needs exploration. I have noticed that people rarely stand directly opposite one another. They are much more likely to stand at an angle. Perhaps this, like the preference for corner seating, reflects the need for psychological escape. When people sit or stand directly opposite one another, it is very difficult to avoid looking at the other person. When they sit at an angle, only a slight movement of the head will remove the other person from view.

I hope this research proves useful to architects. Perhaps some readers may think this an exaggeration, but I believe it is just as important for man to learn about space on this planet as it is for him to learn about outer space. For my part, I have been in enormous cars that lacked ample head-room and foot-room. I have been seated too far away from my friends and too close to strangers on various buses, trains, and aeroplanes. I have seen meetings fail when people were not properly arranged. All these are reflections of a basic ignorance about people and space. I hope that the kind of research I have described here will do something towards remedying this situation.

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TITLE: Factors Affecting the Acquisition and Retention of Conditioned Responses in Planaria

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In this experiment I planned first to study the acquisition of the conditioned response in planaria as influenced by the number of trials given at one time and next to study the retention of a conditioned response as influenced by regeneration and a short interval of time.

Apparatus;

Lighting equipment: I used a 200w, six-hour Photoflood bulb suspended four feet above the planaria in a room of semi-darkness.

Shocking equipment: I used an AC Train Transformer, adjusted to the lowest level at which a contraction occurred 100% of the time. I shaped two pieces of aluminum foil, 1½" x 3" to fit into a circular dish, 5" in diameter for electrodes.

Procedure:

The same classical conditioning method as used by McConnell, Jacobson and Kimble (J. comp. physiol. Psychol., 1959, p. 1-5) was employed in this experiment with one second of shock as an unconditioned stimulus preceded by two seconds of light as a conditioned stimulus.

Twenty planaria were used, with four each in the following five groups: (1) Experimental Group I which received conditioning trials in blocks of 10, 3 days apart; (2) Experimental Group II with trials in blocks of 20, 12 hours apart; (3) Light Control Group; (4) Shock Control Group; and (5) Response Control Group. The planaria of Experimental Group I were given 40 trials each before meeting a sad fate due to evaporation of the water in their dishes. All other planaria were given a total of 90 trials at which time all of the Experimental Group II had reached the criterion of 10 out of 10 responses which was considered 100% success.

Next, two of the previously conditioned planaria were cut transversely and allowed to regenerate until both head and tail portions were able to respond to light (19 days) and then retested. This Experimental Regeneration Group was compared with a Time Control Group of two previously trained uncut planaria and a Regeneration Control Group of two untrained regenerated planaria, after the same interval of time following cutting.

Results:

See Tables I through V at end of text.

Discussion:

Results of this experiment showed definite conditioning in the experimental groups as compared with the control groups. There were individual differences in the rate of conditioning with an average of about eighty trials needed to reach criterion.

The rate of conditioning was faster in the Experimental Group II with trials in blocks of 20, spaced 12 hours apart, than in the Experimental Group I with trials in blocks of 10, three days apart, and also in comparison with the results of Thompson and McConnell (J. comp. physiol. Psychol., 1955) using blocks of 50 spaced 5 minutes apart. Thus, the rate of conditioning is shown to be influenced by the number of trials given at one time and also the spacing of the trials. Further study varying the number of trials and the spacing of the intervals would be desirable to find the best combination of these factors.

There was a high degree of retention in both the cut (90%) and the uncut (97%) as compared by the savings method. The regenerated head and tail portions showed no significant differences in learning or relearning.

While we may conclude that the cephalic ganglia are not necessary for the retention of a conditioned response, we cannot conclude that they are necessary for the acquisition of a conditioned response on the basis of a conditioned response involving the visual mechanism in the cephalic portion of the planaria. To explore this further I am planning to use a non-visual conditioned stimulus such as vibration used by Cornwell and Cornwell (Worm Runner's Digest, 1960) in cut planaria.

While AC current used in this experiment was not lethal to the planaria given totals of about 100 trials and did not seem to affect regeneration after cutting, it seems to have had an adverse effect on the normal process of reproduction by division. At present (May 28), eight weeks after the original testing no individuals have reproduced although one individual started division eight weeks ago.

TABLES

Table I

Group Results

Correct Responses of	Experimental Group I	Experimental Group II	Light Control	Shock Control	Response Control
40 trials	15	12	14	6	7
80	15	23	13	8	6
120	16	25	15	9	7
160	18	29	13	8	5
200		26	16	6	7
240		34	14	8	6
280		36	13	7	6
320		37	11	6	5
360		40	9	5	6

Table II

Individual Results of Experimental Group II

Correct Responses of 10 trials	A	B	C	D
20	6	4	6	7
30	6	5	7	7
40	8	6	7	8
50	7	6	6	7
60	8	8	10	8
70	9	9	10	8
80	10	10	10	7
90	10	10	10	10

Table III

Experimental Regeneration Group

Individuals	Original Training	Retest Head	Retest Tail
C	60	17	21
D	90	15	12
Average	75	16	16.5

Numerals indicate the number of trials needed to reach the criterion (including the last 10 reponses of the criterion).

Table IV

Time Control Group

Individuals	Original Training	Retest
A	80	14
B	80	10
Average	80	12

Table V

Regeneration Control Group

Individuals	Head	Tail
E	70	72
F	60	74
Average	65	73

TITLE: Learning and Regeneration in the Planarian*

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Introduction:

Studies have demonstrated that the behavior of the planarian can be modified, but whether the nature of this modification is learning or sensitization has not been established. Experiments have proved that there is retention of behavioral change through regeneration, but have not been consistent in determining the precise nature of this transmission. In view of present knowledge in this area it seems reasonable to suppose that the entire nervous system of the planarian is responsible for retention of a behavioral change. The present study hypothesizes that planaria can be conditioned and that the retention through regeneration is related to the amount of neural tissue of a conditioned animal which is allowed to regenerate.

Method:

The subjects used were 75 planaria. The apparatus was essentially a duplication of the one used by Thompson and McConnell (1955) expanded to test five subjects at one time. The procedure involved two main experimental groups. Each group was conditioned to criterion using a light as the conditioned stimulus and a shock as the unconditioned stimulus. The subjects in Group E_a were cut transversely anterior to the pharynx and the subjects in Group E_p were cut transversely posterior to the pharynx. This meant that in Group E_a the regenerates came from small heads and large tails and in Group E_p the regenerates came from large heads and small tails. Several control groups were run to determine the effect of time, regeneration, light and shock.

Results:

The hypothesis that planaria can be conditioned was proved. The evidence was presented substantiating that a behavioral change had been produced. It was shown that light was an indifferent agent and continued to be so throughout the trials. The results of the shock control groups demonstrated that an increase of shock trials caused an increase in responsiveness to light. Sensitization caused by the shock did not seem to account for the subjects having reached criterion. The possibility that light and shock sensitization might interact to produce the results was not considered likely. It seemed that conditioning had occurred in the planaria.

* The research reported in this paper was part of an Honors research program performed by Miss Agoston under the direction of Dr. Elinor Wardwell of Smith College. Since the thesis itself was too long for presentation in this journal, the Editor has undertaken to condense it. Realizing that any condensation would fail to do justice to the author's hard work, the Editor has also encouraged her to publish her complete results elsewhere.

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It has been established that there was retention of a behavioral change through regeneration for all the main experimental groups except one. This behavioral change was not due to delayed sensitization to light or shock. Cutting and regeneration did not accelerate the planarian's ability to attain a conditioned response. It seems the second hypothesis is proved; there is retention of a conditioned response through regeneration.

Comparison of the trials to criterion for Groups E_a heads, E_a tails, E_p heads and E_p tails demonstrated that the heads showed more savings than the tails, and that for certain subjects the larger piece showed more savings than the smaller piece. An examination of the trials to criterion for Groups RC_a heads, RC_a tails, RC_p heads and RC_p tails indicated that the same relative order existed between these groups as between the above mentioned experimental groups. It seemed that regeneration affected the relative ability of the regenerates to learn. The head-tail and amount differences found in the main experimental groups might be due to this factor. Although savings of the response might vary with region and amount of body tissue, this effect is masked by the effects of regeneration. There is also the possibility that the use of alternating current might have affected the ability of the various regenerates to learn. Studies on the effect of direct and alternating current will have to be done with larger numbers of subjects in order to clarify this issue. It seems that at this time no final conclusion can be drawn concerning the third hypothesis (that the amount of retention after regeneration is related to two factors: the region of the worm which regenerates and the amount of the worm which regenerates).

Summary and Conclusions:

Research done on learning in the planarian has demonstrated that the behavior of planaria can be modified. Whether the nature of this modification is learning or sensitization has never been clarified. Studies have proved that there is retention of a behavioral change through regeneration, but have not been consistent in determining what factors affect this transmission. The present study was undertaken in an attempt to solve partially these critical issues. There were three hypotheses: 1) Planaria can be classically conditioned; 2) There is retention of the conditioned response through regeneration; 3) The amount of retention after regeneration is related to two factors: the region of the worm which regenerates and the amount of worm which regenerates.

In order to test these hypotheses planaria in the main experimental groups, E_a and E_p, were conditioned to criterion using a light as the conditioned stimulus and a shock as the unconditioned stimulus. The subjects in Group E_a were then cut transversely anterior to the pharynx and the subjects in Group E_p were cut transversely posterior to the pharynx. This meant that in Group E_a the regenerates came from small heads and large tails, and in Group E_p the regenerates came from large heads and small tails. Several control groups were run to determine the effect of time, regeneration, light and shock.

The hypothesis that planaria can be conditioned was proved. Evidence was presented substantiating the hypothesis that a behavioral change had been produced. It was shown that this behavioral change was not caused by sensitization to light or to shock.

The hypothesis that there is retention of the conditioned response through regeneration was also proved for all but one group. It was first established that there was retention of a behavioral change through regeneration for all the main experimental groups except the regenerates of the small tails. The findings on several control groups established that this behavioral change was not due to delayed sensitization to light or shock or to an acceleration in susceptibility to conditioning by cutting and regeneration.

Whether or not the amount of retention after regeneration is related to the region and the amount of the worm which regenerates was not proved. It was demonstrated that for the second learning of Groups E_a heads, E_a tails, E_p heads and E_p tails, the heads showed more savings than did the tails, and the larger pieces showed more savings than did the smaller in certain subjects. An examination of the trials to criterion for groups RC_a heads, RC_a tails, RC_p heads and RC_p tails revealed that the same relative order of learning existed between these groups as between the above-mentioned experimental groups. It seemed that regeneration affected the relative ability of the regenerates to learn, and thereby masked the effects that this research was attempting to locate.

It was also shown that alternating and direct current have different effects on the ability of the regenerates to retain. It seems that at this time no final conclusion can be drawn concerning the final hypothesis.

This study clearly indicates that more research is necessary in the field of learning in the planaria. It has not been established where retention occurs in the body. The effect of regeneration on retention needs clarification. Above all, when further studies are done using shock as a stimulus, the exact effect of the voltage and type of current used must be investigated. It is hoped that the questions raised and the questions answered in this study are of value to future worm experimenters.

"Psychology is as unnecessary as directions for using poison."

Karl Kraus

TITLE: A Study of Classical Conditioning in the Planarian*

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Summary:

The purpose of this experiment was to study both classical conditioning and retention of the conditioned response in the Planarian (D. dorotocephala). It was based on previous work done by McConnell et al. and was an attempt to replicate their results. Five groups of three animals each underwent classical conditioning in which light was the conditioned stimulus and shock the unconditioned stimulus. The procedure followed was that of training the animals through 200 paired trials of CS and UCS (8 training days). They were then cut in half transversely and allowed to regenerate. Following the regeneration period of approximately four weeks, both halves were run for an additional 200 trials in an attempt to determine if the regenerated animals displayed any retention of the CR.

It was found that at least one criterion of learning--a stable change in behavior as a result of experience--was met in the experimental animals. No conclusions can be drawn regarding the retention of the conditioned response in regenerates save that there is some slight evidence that it exists. However, this is far from conclusive.

Discussion:

The results obtained with the experimental group suggest that learning has been established in the Planarian. However . . . this learning, particularly in the testing portion of the experiment, was somewhat erratic. Even so, the results are not too discouraging for the elevations and depressions of the curve become successively higher with time indicating regular growth or increase of the CR as a function of training.

It can be seen . . . that the percentage of CRs obtained during training (and to a lesser degree during testing) by the individual animals of each experimental group show a startling amount of covariation. This suggests the presence of a variable unknown to the experimenters which is influencing the behavior of the subjects in a manner different from that of the training. Curves plotted for each experimental group show that the effect of this variable is regular within groups over days but is random over days for each animal. Thus when one subject's performance improves or declines so does the performance of the other subjects in his group. The effect of this variable is such that the behavior of animals cannot be predicted by the day but a

* The research reported in this paper was part of a Senior research program performed by Mr. Peirce under the direction of Professor John S. Caylor of the University of California. Since the thesis itself was too long for presentation in this journal, the Editor has undertaken to condense it. Realizing that any condensation would fail to do justice to the author's hard work, the Editor has also encouraged him to publish his complete results elsewhere.

whole group's behavior can be predicted to an astonishing degree from the performance of one animal. The effects on all animals on one day are uniform. The influence of this variable might be antagonistic to the variables producing learning; on the other hand, it may be a condition which is conducive to learning. Only further study can determine which. The identification of this variable and the determination of a method for controlling it are problems which must be left for another experimenter. An understanding of it may well result in both improved and more stable performance in the subjects. It seems unlikely that this unknown can be related in any way to feeding, temperature of the water, or time interval between changes of water as these conditions were kept absolutely uniform among the animals in each group and fairly uniform among groups.

Although evidence regarding retention of the CR in regenerated animals was slight, there are, as mentioned earlier, indications that possibly there were some savings which did not manifest themselves greatly due to other causes. It is entirely possible that debilitating effects are being generated by the keeping of animals in finger bowls rather than in their natural habitat. A control group is strongly recommended to determine how great this effect is on local Planaria. It should consist of a group of subjects taken from the stream and treated exactly as the experimental animals were treated except that they would not be exposed to either the UCS or the CS. The members of this group should be kept in individual aquaria and maintained on the same feeding and water changing schedules as experimental animals. It would be advisable to run this control group concurrently with an experimental group in order to bessure that conditions for the controls were identical with that of the experimental animals. The controls should even be placed in the training troughs for the same length of time each day as the experimental animals and made to keep active. The controls should be allowed to "rest" during the four weeks allowed the experimental group to regenerate. At the end of this time, the controls would receive their training. Thus the control group would receive blocks one through eight while the experimental group received nine through sixteen. A comparison of the curves obtained by the controls with those obtained by the experimental animals during training would then be made. A significantly large difference in performance between the two could then be ascribed to the debilitating effects of the laboratory environment. Although this control was not accomplished, it was felt by the experimenters that "fresh" animals seemed to perform better than those who had been taken from the main aquarium.

In the event that the lab environment is shown to have debilitating effects on animals, it would be beneficial to devise a method for returning the cut subjects to their native habitat for regeneration.

A time control group was included in this study by the simple expedient of merely not cutting one trained animal in each of two experimental groups. These animals (E-3M and E-4R) did not demonstrate any better performance during the testing cycle than did the regenerates. This also points to the possibility of deleterious effects resulting from captivity and not to traumatic effects of being cut.

In summary, it might be said that this attempt at replication yielded results generally consistent with those obtained by McConnell; they in no way contradict his studies. It cannot be stated conclusively that the phenomenon

of classical conditioning has been established in the subjects of this experiment. As noted earlier, one criterion was met, but, as the extinction control group was not attempted, the second was not. As to savings, this study can neither confirm nor deny its existence; it can merely suggest that further work is necessary using this study as a beginning point. The significant difference between the means of days three and eleven . . . suggests that some savings of the CR was manifested by the regenerated animals. It seems probable that variables not under experimental control are operating to obscure retention. It might be noted here also that there were no significant differences found between the performances of regenerated head and tail sections.

Because of the time limitations imposed by the ending of the academic year and the difficulties encountered at the beginning, this experiment is actually only a pilot study which, it is hoped, can serve as a guide and a goad to further research on this fascinating and tough but tender little beast, the Planarian.

"This intelligence-testing business reminds me of the way they used to weigh hogs in Texas. They would get a long plank, put it over a crossbar, and somehow tie the hog on one end of the plank. They'd search all around till they found a stone that would balance the weight of the hog and they'd put that on the other end of the plank. Then they'd guess the weight of the stone."

John Dewey

TITLE: On Not Using Psychology in Psychology

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> "The manner in which animals learn has been much studied in recent years, with a great deal of patient observation and experimentation. . . . One may say broadly that all the animals that have been carefully observed have behaved so as to confirm the philosophy in which the observer believed before his observation began. Nay, more they have all displayed the national characteristics of the observer. Animals studied by Americans rush about frantically, with an incredible display of hustle and pep, and at last achieve the desired result by chance. Animals observed by Germans sit still and think, and at last evolve the solution out of their inner consciousness."
>
> Bertrand Russell, Philosophy
> New York: Norton, 1927, p. 30.

Russell's facetious point is still, three decades later, more truth than fiction. One can dismiss it by invoking the shaping effect of the apparatus employed on the theory; or perhaps more subtle mechanisms are implicated by this astute indictment. The purpose of this paper is to explore the latter possibility.

I. Introduction and thesis

In recent years certain empirical developments have suggested that a critical re-examination and revision in the methodology of experimental psychology are in order. The indications come from diverse areas, but their unanimous conclusion is unequivocal: that more rigorous control of experimental conditions and greater standardization of procedures, measurements, and terminology are necessary and perhaps not even sufficient if psychology is to remain a meaningful endeavor.

The problem of inadequate procedural control and rigor is not, of course, just now dawning upon psychologists. The more meticulous experimentalists have been quite self-consciously concerned with methodology (see, for instance, Melton, 1936) ever since the days when scientific psychology had to fight for academic respectability. It has often been the case that uncontrolled variables turned out to be crucial factors in experimental situations. Three examples that come readily to mind are: word recognition as affected by personal values (Postman, Bruner, and McGinnies, 1948), in which the frequency of exposure to the stimuli was later found to be a crucial uncontrolled variable (Goldiamond and Hawkins, 1958; Solomon and Howes; 1951; Solomon and Postman, 1952); the previously-established relationship between response strength and time of deprivation in rats (Perin, 1942; Yamaguchi, 1951), which shrank to negligible proportions (at least for the situation tested) when the neglected factor of competing responses was effectively held constant (Cotton, 1953); and the socially intriguing demonstration of varying viability concomitant with changes

in the social milieu of the home cage, which turned out to be attributable to so mundane a factor as temperature changes produced by cell-mates and absent in isolation (King and Connon, 1955). In another realm, clinical researchers have long been under fire from their harder-headed colleagues for the frequently-loose design and control of their investigations.

It is evident, then, that the problem of control has always been with us. What is new in the converging indications to be discussed here is the operation of seemingly minor, irrelevant factors in producing large effects, both quantitative and qualitative. Though the importance of objectivity and standardization have long been recognized, the order of required control suggested is formidable, if not downright dismaying. In the light of the evidence to be marshaled, a variety of classes of research, encompassing conservatively 50% of experimental psychology, are suspect--sight unseen--because of their methodology. The topics to be considered now in support of this outlandish proposition are: extra-sensory perception, verbal reinforcement, projective testing, interviewer bias, early experience studies, and direct evidence.

II. Indicators of the need for revision: Extra-sensory Perception

Perhaps the most amazing aspect of ESP is that after its many years on the experimental scene there is so little unanimity of opinion concerning its very existence. Anecdotal, demonstrational, and experimental evidences are abundant, and some psychologists are convinced of the validity of the phenomena (Eysenck, 1957; McConnell, 1957); yet most scientists remain unconvinced (Gardner, 1957; Kennedy, 1939). Their usual objection is that no unequivocally well-controlled and repeatable demonstration of the occurrence of extra-sensory perception has yet been carried out (sic). A fundamental methodological question is implicit here: that of deciding when control in an experiment is adequate. Theoretically there are an infinite number of variables in any situation; the decision as to which of these may affect the outcome, and therefore need to be controlled, is the experimenter's prerogative.

The task of the advocates of extra-sensory perception is especially difficult for two reasons: 1) the esoteric nature of the phenomenon itself biases scientists against its acceptance into the body of verified knowledge, and 2) cues of such seeming unimportance have in some studies proven to be the actual determinants of the supposedly pro-ESP results that the critic is led to be abnormally suspicious. In an extensive review of the methodology of ESP studies, Kennedy (1939) points out that some positive results are attributable to slight auditory and visual cues transmitted unwittingly from the "senders" to the "receivers". For example, the same Kennedy (1938) tested the hypothesis that "unconscious whispering" may serve as a communicative cue. Blindfolded, naive senders were placed at the focus of one parabolic reflector and the receiver at the focus of another. Under these conditions the receivers were able to guess cards at a significantly better-than-chance level; but when the reflectors were removed, the receivers' "extra-sensory perception" dropped to chance level.

Another suggestive study cited by Kennedy is the famous and amusing story of "Clever Hans", the mind-reading horse. Hans aroused a storm of public interest by his remarkable ability to perform mathematical calculations and indicate his solution by head-nodding and hoof-tapping. Upon investigation of the case, Pfungst (1911) found that the horse was in fact responding to slight and inadvertent sensory cues from his trainer. This is at the same time instructive

and reminiscent of the man who dismissed his dog's chess-playing ability with the remark, "So what? A good player can beat him easy."

Whether or not ESP phenomena are "real" is actually somewhat tangential to the body of experimental psychology. The most important and fascinating outcome of ESP research is the demonstrated effectiveness of minimal, apparently negligible cues in transmitting information and in influencing behavior. The implications of such studies as those of Kennedy and Pfungst can hardly be overemphasized; in the words of one group of writers: "Kennedy's studies also point up . . . (that) . . . all of us, apparently, are constantly attempting to influence the people around us by means of sounds and movements we are unconscious of making. Correspondingly, all of us make some unconscious use of the cues presented to us by the people around us" (McConnell, Cutler, and McNeil, 1958). This is a bit Sunday supplementish, perhaps, and the suggestion of "attempt" is certainly gratuitous, but the point is nonetheless well taken.

Are the possibilities for such uncontrolled communication present in any psychological experiments? Only the obtuse will deny it. Does this vitiate these experiments, or at least render their conclusions suspect? Let him who doubts it go his errant way without our blessing. In a later section of this paper, we shall discuss an experiment (Warner and Raible, 1937) in which this very type of cue played a critical role.

III. Indicators of the need for revision: Verbal Reinforcement

In a recent but already-classic experiment, Greenspoon (1955) demonstrated clearly the feasibility of employing verbal reinforcements to control another person's verbal output without that person's awareness of the effect. Greenspoon reinforced his subjects' utterances of plural nouns in free-association by saying "mm-hmm" and found that their emission of that class of responses doubled. In subsequent extinction, the proportion dropped to its original level. As nearly as Greenspoon could ascertain, 65 of his 75 subjects were not aware (as guaged by verbal report) of the response-reinforcement contingency.

Greenspoon's study is but one of many on the issue of the operant conditioning of verbal behavior. Krasner (1958) reviews 31 such studies, varying in type of reinforcer used, response class reinforced, and several lesser dimensions. Without going into detail, it may be noted that the bulk of the studies have been quite successful in achieving the desired effect. Krasner divides the reinforcing stimuli into three general types: verbal, gestural, and mechanical devices. Other verbal reinforcers used besides "mm-hmm" are "good", "I see", etc.; on the negative side, "huh-uh" has been found to decrease the frequency of plural nouns (Greenspoon, 1955). In a study by Verplanck (1955), each of his 24 subjects increased their rate of verbalizing statements of opinion when the examiner reinforced such statements by paraphrasing or agreeing with them.

Gestural cues that have been employed are head shaking, head nodding, smiling, leaning forward and paying attention, etc. These devices have typically been combined with verbal cues, but they do seem to enhance the reinforcing effect. Hartman (1955) found a head shake to be an effective reinforcer, but a head nod was not. Of interest here is a study by Greenspoon (unpublished, cited in McConnell, 1959), in which therapists were found to provide selective reinforcement for their patients' verbal utterances by the amount of attention and interest that they (the therapists) displayed.

A new note creeps in with the last-cited experiment, for here the reinforcing agent performed that function inadvertently, rather than in a preset, contrived manner. Here, then, is another kind of communication that may occur in the experimental situation, in addition to that revealed in the extra-sensory perception research. There the information transmitted was specific and experiment-relevant. Here, the "information" consists of generalized conditioned reinforcers, selectively exercised in a manner that differentially affects the subjects' responses.

IV. Indicators of the need for revision: Projective Testing

Allusion was made earlier to the constant state of siege under which clinical psychologists must labor. This is not meant to imply that such aversive stimulation has had any influence on the behavior of the clinician in the administration of projective tests. The results of years of parametric studies indicate clearly that ". . . the procedure that many clinicians hoped would serve as an X-ray proves, on close examination, to function also as a mirror reflecting impartially S, E, the situation, and their interactions" (Masling, 1960, p. 81).

The evidence that leads the reviewer to this conclusion is so overwhelming as to need no further argument here. It is clear that projective test results--that is, the behavior of the subject--are affected by several classes of variables, often seemingly trivial. Among the variables which have produced significant differences in response are: instructions; pretest conditions; sex, presence, warmth or coldness, and other characteristics of the examiner; and various "atmosphere" factors. Several studies have found the subjects' projective responses to be susceptible to reinforcement by postural, gestural, facial, and verbal operations.

Exactly what this implies for projective testing prescriptively is debatable. It has often been suggested that the administration of tests ought to be standardized, insofar as is possible. This depends, however, on whether one feels that it is desirable to divorce the interpersonal relationship from projective test administration. According to one writer (Schafer, 1954, p. 6), as cited by Masling (1960), the interpersonal situation ". . . is not an evil. It should not be striven against. As in psychoanalytic technique, this relationship must be regarded as inevitable, as a potentially significant influence on the patients' productions, and as a possible goldmine of material for interpretation". Masling, agreeing with this position, states that "instead of trying to eliminate the interpersonal and situational influence E might better make a more thorough search of his own attitudes and of S's sttitudes toward the test and the situation" (p. 82).

The conviction of the present writer is that such an approach consigns projective tests to the clinician's arsenal of intuitive weapons, and precludes their development as an objective assessment tool. This may be a permanent consignment, or it may last only a hundred years or so, until all the variables under consideration have been investigated and delineated sufficiently in all their interactions to permit the clinician to take them into account adequately.

Rather than pursuing this issue further, however, let us briefly note the obvious relevance of this area of investigation to psychological research in general. Here, hopefully, there will be little debate about the desirability of controlling conditions. So many of the studies reviewed by Masling are

relevant and telling in the present context that it is difficult to make a selection. Suffice it to say, therefore, that Masling groups the studies he reviews into four overlapping but on the whole separable categories: the influence of method of administration (especially instructions), the influence of the testing situation, examiner influence, and subject influence; and that each of these has been shown convincingly to alter the subjects' behavior considerably.

V. Indicators of the need for revision: Interviewer Bias

One of the major problems of any organization that bases its operations on survey data is that of the interviewer as a variable in the elicitation and collection of data. Because the research in this area demonstrates the same point as do a number of the studies on the examiner effect in projective testing, no further discussion of this work is required. It is worth pointing out, though, that the body of research on interviewer bias provides a wealth of substantiation for the proposition of experimenter influence on the subjects' responses. Here again the attitudes, personality, knowledge, and personal characteristics of the examiner have proven to be potent factors. Hyman (1954) has done an extensive review of the study of interviewer effect and its meaning for social research. It is only fair to acknowledge that the problem of interviewer bias has long been a salient one in the minds of social researchers and has received its due share of attention.

VI. Indicators of the need for revision: Early Experience Studies

The effect of early experience on later behavior would seem to be a most obvious area for empirical investigation. Yet, although there had been scattered efforts (e.g., Hunt's (1941) studies of hoarding), only in the last ten years has a concerted attack upon this problem been mounted. The time course of activity devoted to early experience studies can be related to theoretical lags and leaps, with Hebb's (1949) neurophysiological theory of development providing the major modern impetus. The difficulty of conducting research of this type has been carefully analyzed by King (1958), and is an important methodological lesson in itself.

The two major dimensions of early experience studies are the type of environmental manipulation performed and the aspect of later behavior tested. Of special interest for present purposes is a variety of experiments demonstrating significant and in some cases surprisingly strong interactions between these independent and dependent variables. One group of studies has investigated the effects of variations in early handling of rats, and found marked differences in later learning (Bernstein, 1957; Levine, 1956), emotionality (Levine, 1956; Tedeschi, 1959; Weininger, 1956), and consummatory behavior (Levine, 1957). Hebb's speculations have inspired studies testing the consequences of differential enrichment of impoverishment of the early perceptual environment. Significant effects have been demonstrated on later problem-solving (Bingham and Griffiths, 1952; Forgays and Forgays, 1952; Hymovitch, 1952; Thompson and Heron, 1954), emotional responses (Melzack and Scott, 1957), and problem-solving flexibility (Luchins and Forgus, 1955). Cooper and Zubek (1958) found differential problem-solving effects of environmental variations in bright and dull rats. Levine (1958) has provided evidence of detrimental effects of late weaning on adult avoidance learning. Denenberg and Naylor (1957) have shown significant effects of early food schedule on performance on a discrimination task involving a food reward, and have interpreted this as a drive effect.

Certain implications of the foregoing, which may already be apparent, have been cogently pointed out by Christie (1951). Distinguishing between "experimental naivete" and "experiential naivete", he contends that the frequently encountered phrase, "the subjects were N experimentally naive rats of such and such a strain" implies ". . . either that the animals' previous experience is of no importance whatsoever, or that it is so uniform that every 'experimentally naive' rat is a standardized little mechanism subject to exactly the same pre-experimental influences in laboratories at California, Iowa, or elsewhere" (p. 237). Christie goes on to specify several pre-experimental variables that are typically uncontrolled, namely: the source, maintenance schedule, caging, and handling of the animals. In the light of the early experience studies reviewed in this section, the differences in these factors from laboratory to laboratory and within a given laboratory can be seen as sufficient to contribute appreciably to the inconsistencies that have for so long plagued many types of animal research. It should be recognized that such differential effects as we are considering may be of a qualitative as well as of a quantitative nature. An example of such a qualitative effect will be provided in the next section (Christie, 1952).

VII. Indicators of the need for revision: Direct Evidence

The plea for a more sophisticated psychology of the experimental situation has been voiced before, although usually only in the form of addenda to experimental reports. We have seen that Christie (1951) recognized and discussed one class of extra-experimental variables, namely the prior history of animal subjects. Rosenthal (1958a, 1958b) is concerned with another class: the relationship between the subject and the experimenter. To account for some results which are unfortunately not available to the present writer at the moment, Rosenthal speaks of what he calls "unconscious experimenter bias", which will be seen as one of the major phenomena that have been emphasized in the foregoing. "How many experiments with human or perhaps even with animal Ss have had their results influenced by the phenomenon of unconscious experimenter bias we will doubtless never know" (1958b, p. 662). Rosenthal claims that his earlier paper (1958a)". . . showed that the chances were 99 out of 100 that E was able to and in fact did influence his Ss to give the responses which E 'needed' to get. Furthermore, E was unaware of the operation of this biasing factor, which came to light only in a most serependipitous manner" (1958b, p. 662).

A small number of scattered studies provide relatively "direct" evidence for the potency of the biasing factors that this paper has considered. These studies will be abstracted now along with the particular variable that they illustrate.

Sensory cues

Warner and Raible (1937) tested near-threshold judgments of weight discrimination. They obtained extra-chance results when the experimenter knew the correct choice, but only chance results when the position of the correct choice was unknown to E. The writers conclude: "Critical consideration of the conditions leads us to believe all modalities beyond suspicion except hearing . . . True, it seems far-fetched to suppose that such cues could in any way be construed so as to aid in the making of correct judgments. It is also true that our subjects (and there seems no reason to doubt their word) were entirely

unaware of any assistance from such sources or from any sources other than the feeling of the weights themselves" (p. 50).

E characteristics

Birney (1958) failed to replicate a previous study which had found a relationship between achievement motive and task performance. When he substituted a student E for the faculty E that he had first used, however, the anticipated relationship showed up. It is a bit unclear whether the difference was due to status or familiarity, but in either case, Birney's conclusion is valid: that "The possibility is thus suggested than an E effect has been present in many previous studies without sufficient appreciation of its role" (p. 134).

E attitudes and expectations

Maier somewhat parenthetically relates a striking anecdote illustrating "the role of the experimenter in influencing the behavior of animals" (p. 375). Two research assistants of his were working in adjacent rooms on similar problems. Whereas one obtained "the usual number" of fixated responses (over 50%) among a large number of rats, the other failed to obtain a single fixation. Their procedures and routines appeared identical in every way. It was later discovered that the second assistant's sympathy for his subjects' sufferings led him to treat them more affectionately between trials than did other researchers.

Dember (1960) cautions against another E bias which may occur in psychophysical (and other) experiments: "The experimenter, expecting a hypothesis to be verified, innocently chooses the values of the stimuli to be presented in accordance with his expectations of about where the two threshold values should fall. . . . . Sure enough, the two thresholds do differ, but not necessarily as a result of the intended experimental manipulation" (p. 40). As evidence for this, Dember mentions a replication attempt (Riggs, Cornsweet, and Lewis, 1957), which was unsuccessful because of minor procedural variations.

Pre-experimental variables

It has been a common finding in latent learning experiments that hungry, non-thirsty rats which learn to go to the food-containing side of a T-maze rather than the irrelevant water-containing side, continue to go to the food side even when thirsty and not hungry. Christie (1952) found, however, that in contrast with this, rats which have a history of food and water deprivation are able to respond appropriately when drive conditions are reversed. These animals, though, perform only at a chance level when the original drive conditions are reinstated. Christie explicitly concludes that "Richer early experience in this case quite definitely led to qualitatively different behavior" (1951, p. 336).

One wonders also what subtle and mischievous variable caused Walk (1958), in a very careful replication of an experiment by Forgus (1955), to achieve significant results, as had Forgus--but in the opposite direction.

S attitudes and expectations

Orne (1959) has performed a series of experiments which seem to indicate that hypnotic behavior is largely shaped by the subject's knowledge and/or

expectations concerning hypnosis. In one study, the experimental group was told that catalepsy of the dominant hand is a common feature of trance behavior. Five out of nine subjects in this group later exhibited this symptom under hypnosis, as compared with none in the control group, which did not receive the specific suggestion. This and similar findings led Orne to conclude that ". . . the demand characteristics of the experimental procedure may be a significant determinant of subject behavior" (1959, p. 298).

Somewhat related to this, and a fitting closing example, is the classic research on factors in productivity conducted at the Hawthorne plant of the Western Electric Company. A series of variations in work pacing, wages, lighting, and the like, all produced increases in productivity. It soon became apparent that social, interpersonal, and attitudinal factors were important uncontrolled influences in these changes: "From the attempt to set the proper conditions for the experiment, there arose indirectly a change in human relations which came to be of great significance" (Roethlisberger and Dickson, 1939, p. 59).

VIII. Conclusions

What is the upshot of all this, assuming for the moment that the issues are as important as the writer believes? The faint of heart may cringe at the havoc that may be wrought by such considerations. The tough-minded experimentalist will simply hitch up his belt and plunge in anew, but perhaps with a more searching and critical eye. Actually, the recognition of previously-neglected variables is a healthy sign at any stage in any science. We can do no better here than to quote Hyman (1954), who has made this point quite cogently:

"Let it be noted that the <u>demonstration</u> of error marks an advanced stage of a science. All scientific inquiry is subject to error, and it is far better to be aware of this, to study the sources in an attempt to reduce it, and to estimate the magnitude of such errors in our findings, than to be ignorant of the errors concealed in the data. One must not equate ignorance of error with the lack of error. The lack of demonstration of error in certain fields of inquiry often derives from the nonexistence of methodological research into the problem and merely denotes a less advanced stage of that profession" (p. 4).

Psychology is hardly at "an advanced stage", but a careful investigation, characterization, and thereby control of these new sources of error is potentially the biggest step <u>toward</u> an advanced stage that psychology has yet taken.

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THE WORM'S-EYE POINT OF VIEW

(Editor's Note: Beginning with this issue, we will share with you many of the letters which our group receives. The letters will be edited in many cases; in particular, we will remove accolades where they serve no other point than making us feel good. We will also append comments where we have wit enough. The title of this hopefully regular feature is taken from Ernie Pyle's HERE IS YOUR WAR: "I write from the worm's-eye point of view.")

FROM: H. M. B. Hurwitz
Department of Psychology
Birkbeck College
University of London

Keep glowing and wriggling, please. Pity you have to use the present duplicating system. My Vol. I is rapidly fading - but perhaps that is part of your (diabolical) scheme. Has anyone explored what science would be like if all published papers faded into limbo after, say, ten years? It would make many a PhD student very happy.

(As you can see from this issue, we have taken your advice, but reluctantly. After all, who can hold us responsible for Vol. I ten years from now if the thing can't be read?--Ed.)

FROM: Max Braverman
Vivarium Building
Zoology Department
University of Illinois
Champaign, Illinois

You expressed concern about the Chicago High School students who wrote to you for instructions, etc. so that they could prepare a project for their Science Fair. I saw the displays of a number of these students last week at the Illinois Jr. Academy of Science meetings here, at Champaign. Generally, I was overwhelmed. Most of those kids had a firmer grasp on the ideas of experimental biology than did my fellow students in the department of Zoology. This despite, or perhaps because, they were limited in the facts they knew to their own special projects. There were about five students, or pairs of studetns who had repeated your findings with planaria, who were demonstrating their conditioning apparatus...one who had hundreds of planaria and was selling them at ten cents each. I spoke to him because, using Carolina Supply's dorotocephala, I could get eggs, but never fission. It was a real pleasure to talk to this fellow. He knew his stuff, and was very willing, unlike most conservative biologists, to make a few guesses that went beyond his data. Most of the young people I talked to did experience the experimental difficulties you were concerned about, but worked their way through them, and came out, I would guess, with the feeling that they could, using their own resources solve the problems -- which to my way of thinking is the reward people playing with animals have instead of the high salaries of our commercial contemporaries.

FROM: Ruth A. Ziegler
3107 Crockett Avenue N.W.
Roanoke, Virginia

This is a letter which should have been written many months ago. I am a sophomore at William Fleming High School in Roanoke, Virginia. I recently attended National Science Fair in Indianapolis, Indiana. This was one of the biggest and best experiences of my life. However, it would not have been possible without your help. This is where the explaining comes in.

One of my friends had written you coneerning your work with planaria. She had planned to do a project on them for Science Fairs, but discovered that she was unable to work it into her schedule. About this time we found out that I was planning the same project. So, to save me time and trouble, she gave me the material she had already obtained from you and forwarded all the rest as it came.

I began my work in September, 1960 and entered the Roanoke City Fair in March. I came out on top in Girls' Biology and went on to Regional. From there I went to NSF and took a fourth place. By the way, I gave you and Miss Jacobson credit in my research paper, so you werernot entirely left out.

So, thank-you all for your round-a-bout, but wonderful, help in teaching me to "teach wee beasties". I do hope you will not be offended because of my negligence.

FROM: Donald Kirsch
9429 S. 83rd Ct.
Oak Lawn 4, Illinois

Late last fall, my associate, LeRoy Neitzel, and I wrote you a letter, asking for some information about your experiments on planaria. In your return letter, you asked us to tell you about our results.

Your information was most helpful in carrying out our experiment and gave us a basis for our work. It saved us many hours of finding the best solution to simple problems; such as dimensions of the trough, etc.

Thank you also for sending us the second "Worm Runners Digest". It answered several questions we had about the electricity used.

Our project was quite a success at our school Science Fair. We received a first place award and the award for the most outstanding project of the Fair. The judges thought it was very original and interesting.

Because the conditioning process was so time-consuming, we devised an automatic switch controler. It turns on the lights for two seconds; the current is turned on for one second, then both are turned off. There is a time interval of about three seconds, then the cycle is repeated continuously. Using this method, we cannot record the reactions but the planaria is still building up a conditioned response. The controler is very convenient and saves many long hours. However, we have not had enough time to test it for flaws.

We have also had a problem with getting a contraction out of the planaria when the dimensions of the trough are according to those used in your experiments. It seems as though the electrodes are too far apart (14 inches) and the planaria do not respond when a six volt D.C. power source is used.

In order to get a reaction, we added wires to the electrodes and put other electrodes on the wires and then put these electrodes in the trough. This shortened the distance between the electrodes and the planaria then reacted when shocked. This revision seems to work all right but it doesn't look so hot and the judges prefer a neat appearance on projects. Also we know that you obtained results by having the electrodes at this distance apart and we therefore should be able to duplicate the set up.

Thank you again for your time and information. Please write and tell us what we can do to give the beasties a better shock when the electrodes are 14" apart.

(We did write and give Don the information requested. For our latest thoughts on how to condition worms rapidly, see the article by Paul Cornwell in this issue. --Ed.)

FROM: Richard H. Earhart
Baldwin-Wallace College
Berea, Ohio

Thank you for your letter of May 11th regarding my work with mimosa. Since I am leaving Baldwin Wallace after this quarter I have been very busy getting the quarter wound up. As a result I haven't had much time for correspondence.

Actually there isn't too much to tell about the experiments. I did the experiments a number of years ago and never wrote them up. I did make motion pictures of some of the essentials of the experiment though. They can be duplicated fairly readily though.

For the natural stimulus I used a brushing of the leaves. The particular response was the folding of the leaves (not the drooping stalk). The response chosen has a faster recovery time than the stalk and seems to be a more stable characteristic. A one hour inter-trial interval was used. A shorter time cannot be used, a longer might be better- I haven't investigated the longer times.

For the neutral stimulus I used, because it was available, a "Penetray" heat lamp. The lamp was placed 15-18 inches from the front to be stimulated. Other fronds equidistant served as controls. The distance is rather critical--too close and the plant is damaged--too far and it will not give a stable CR (?). The lamp is turned on for a period of two minutes prior to touching the leaves.

The response recorded on film shows the two tip pairs of leaves folding tightly together. The response only occurs on the experimental fronds not on the controls. Sometimes on other plants more than just the tip leaves are affected and sometimes it is the tip ones. It is better to photograph the process

rather than to rely on visual inspection since it isn't quite as startling as having a plant speak.

All plants tested under these conditions show the response in 3- 8 trials!! Very fast modification. You might investigate one rather startling finding. One and only one plant showed a reaction to a constant one hour time interval without a heat lamp. I'm afraid I can't offer any suggestion as to why others don't show the reaction. Some uncontrolled experimental variable undoubtedly.

Another experiment repeated here, originally done in England and reported in some biology books but never with an original source given, is to take oat seedlings planted in regular flower pots. Laid on their side, they showed a geotropism or possibly a response to light - at least they grew up. Periodic reversal for 3 - 10 days with the final day a non-reversal caused some plants to grow down on the non-reversal day. Pictures were obtained of the plants but a student lost the sole copy of the experiment. Not all plants showed the reaction so there are some uncontrolled variables that need investigation.

A third variation uses the "sleep reaction" of mimosa. An immediate reversal of night and day through artificial light caused a gradual reversal of behavior. Apparently it is not a response to light alone.

About 1924, an East Indian named Bosé claimed to have discovered a nervous system in a plant (mimosa naturally). As far as I have been able to find out no-one else has seen it. Probably the Venus fly trap would be a better plant to use since its NS hasn't been discovered yet.

Thank you for your interest in the plant behaviors. Go ahead and try out the experiments if you wish and write them up. If I can be of any further help you can contact me at Oregon Fairview Home, Salem, Oregon after July 18th. I would be happy to hear of your results.

(The editorial in the last issue of the DIGEST contained an admonition to all and sundry that plants might be able to learn. Soon after publishing that issue, we heard of Earhart's work and wrote to him about it. We print his reply in full. At the moment, we are attempting to replicate his study with mimosas and heat lamps. Our results will be presented in a future issue of the DIGEST, positive or negative. Anyone who wishes to write to Professor Earhart should note his new address, mentioned in the last paragraph of his letter. --Ed.)

FROM: Arsene Okun
64-33 99th Street
Forest Hills 74, New York

You may be interested to hear that, as regular contributor to the VOICE OF AMERICA, German Program, I did a 5-minute broadcast on your work with flatworms on May 24, 1960. It may also interest you to hear that, as U.S. correspondent of the DEUTSCHER FORSCHUNGSDIENST, Bad Godesberg, leading German science news and feature service, I reported on your work about two months ago. This portion of my copy was, however, not used and I have a hunch that political considerations may have had something to do with it.

(This is an election year, isn't it? Oh, well.... --Ed.)